The Electoral Migration Cycle

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Abstract

This paper proposes a new test of Tiebout sorting that relies on the exogenous time structure of recurrent local elections. The test is based on the idea that competitive elections represent periodic perturbations to the Tiebout equilibrium of local public good provision and allocation of households to communities, so that their schedule should affect the timing of households' sorting decisions. On the other hand, internal migration flows that have nothing to do with the demand for public goods over which localities vote recurrently ought to be orthogonal to the timing of elections in a reduced-form migration equation. I exploit the staggered schedule of mayoral elections in Italy to analyze migration, elections, and public budget data across several thousands of municipalities, and find evidence of a systematic influence of the electoral calendar on the timing of sorting decisions.

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1 Introduction

The idea of mobile households sorting across localities according to their preferences for public services, or "voting with their feet" (Tiebout, 1956), has played an influential role in theoretical and applied work in urban economics for decades (Oates, 2006). However, to what extent Tiebout sorting mechanisms actually achieve an efficient provision of local public goods and an optimal allocation of households to communities has been shown to depend on circumstances including the availability of Lindahl pricing in environments of heterogeneous communities (Ross and Yinger, 1999) and the objective function of local jurisdictions (Jehiel and Lamy, 2018). From an empirical point of view, the role of local public goods in influencing households' internal migration decisions besides the well-established 'push and pull' factors from labor market conditions remains unclear (Dowding et al., 1994). Part of the scepticism about the practical relevance of Tiebout sorting seems to be due to model specification and estimation difficulties: identifying the effect of decentralised fiscal policy on households' location decisions has proved problematic, and thwarted by endogeneity, reverse causality, and measurement issues (Kuminoff et al., 2013).

In fact, the rare circumstances where the provision of local public goods changes exogenously in a quasi-experimental setting are increasingly seen as the ideal conditions for testing the key predictions of the Tiebout model in terms of capitalization of public amenities into property values and stratification of communities via direct migratory responses.[1] Banzhaf and Walsh (2008) use a difference-in-differences approach to test whether the entry and exit of polluting facilities have an impact on population density (through the number of individuals leaving and entering a community) and mean incomes (changing mix between richer and poorer households) of evenly-spaced 'constructed' neighborhoods in California, and find evidence of out-migration and impoverishment of communities when the public good air quality deteriorates. Chay and Greenstone (2005) and Greenstone and Gallagher (2008) use quasi-experimental approaches to evaluate the welfare impact of mandated federal environmental policies on US local governments, and provide evidence of capitalization of environmental quality

[1] Epple and Sieg (1999), Epple et al. (2001) and Sieg et al. (2004) estimate general equilibrium models of sorting across a finite number of fixed-boundary communities where endogeneity of community-specific amenities arises because the unobserved component of local public goods might be correlated with housing prices and household expenditures, and use functions of the income rank of communities as instruments. The earlier approaches to testing the Tiebout sorting hypothesis are reviewed in Ross and Yinger (1999).

into housing values in hedonic price models that allow for individual preference-based self-selection to locations. Rhode and Strumpf (2003) test instead the long-run Tiebout prediction that the secular decline in mobility costs should increase preference heterogeneity and policy variation across communities. They find no support for those hypotheses on samples of US municipalities and counties, thus challenging the view that community choice be primarily driven by Tiebout incentives. Borge et al. (2014) exploit a partial fiscal decentralization reform in Norway that allowed greater local discretion in the use of public resources, and find higher responsiveness of the local public sector to local demand as well as more intense population sorting after the reform. On the other hand, a recent and growing literature focuses on the impact of discrete changes in local income taxation schedules on the location decisions of high-income taxpayers, generally uncovering large elasticities of internal migration flows to local income tax policy differentials (Schmidheiny and Slotwinski, 2015; Basten et al., 2017; Martinez, 2017; Moretti and Wilson, 2017; Agrawal and Foremny, 2019).

This paper puts forward a novel, reduced-form test of the Tiebout sorting hypothesis that relies on the staggered time structure of local elections as an exogenous and recurrent perturbation to the equilibrium allocation of households to communities. The test is built on the idea that since periodic competitive elections have an impact on the provision of local public services, the calendar of local elections should influence the timing of sorting processes. At the same time, internal migration flows that have little to do with the consequences of elections in terms of local public service provision should not be affected by the timing of local races.

The idea that the periodicity of elections can have an influence on the trajectory of aggregate economic variables (output, unemployment, inflation, and nominal exchange rates) and of public policy (public expenditure, taxation, and regulatory measures) has a long tradition in economics.[2] Nordhaus (1975) was the first to formally argue that the fact that "the government be chosen in periodic competitive elections" (Nordhaus, 1975: 185) can induce incumbent politicians to exploit nominal rigidities and voters' naivety (myopia, adaptive expectation formation, and retrospective voting) and manipulate short-term macroeconomic trade-offs to engineer 'political business cycles' of low pre-election unemployment followed by after-election inflation. Rogoff (1990) later provided

[2] Drazen (2001) reviews the voluminous theoretical and empirical 'political business cycle' literature that appeared in the previous quarter century.

a rational, dynamic signaling interpretation of the observed effect of election schedules on the economy, pointing to the strategic timing of fiscal policy by competent politician types trying to separate from incompetent types by switching to more salient public consumption expenditures and away from investments right before the elections (political budget cycles). Starting with Rosenberg (1992), a strand of the subsequent literature investigated the presence of such cycles on the larger number of observations that are found in local government data, as in the recent empirical analyses of Akhmedov and Zhuravskaya (2004), Baleiras and Da Silva Costa (2004), Veiga and Veiga (2007), Dahlberg and Mork (2011), Foremny and Riedel (2014) and Englmaier and Stowasser (2017).

In practice, testing whether the timing of elections has an influence on the timing of household sorting requires high-frequency internal migration data and an exogenous structure of decentralised government elections. The former makes it possible to monitor the trajectory of migration rates in the vicinity of the date of the election, and the latter allows an interpretation of the election date in a given locality as a randomly assigned treatment. I rely here on a large dataset of Italian municipalities (the about 7,000 local authorities - *Comuni* - located in continental Italy) and exploit the availability of monthly municipal-level migration data along with the staggered schedule of mayoral elections occurring every fifth year. In particular, I analyze the trajectory of migration in the proximity of the three largest election waves of the past two decades involving about 4,000 municipalities and taking place on June 12, 2004, June 7, 2009, and May 26, 2014. Importantly, I can use the set of authorities not holding an election during those years (around 3,000 municipalities) as the control group in a difference-in-differences approach, where the electoral treatment can be taken as quasi-randomly assigned due to the fact that the causes of the staggered election schedule date back several decades.

The results of the empirical analysis can be briefly summarised as follows. First, difference-in-differences estimation on monthly migration data around those three large elections provides evidence that the timing of municipal elections influences the timing of households' migration decisions. After accounting for seasonality of the sorting process and using the municipalities that do not hold elections in those years as the control group, average migration rates in the months following the date of the election turn out to be around four to five percent higher than in the months preceding the date of the election in the localities that actually held municipal elections, with the estimated effect standing a number of robustness checks on different samples and model specifications. Second, I investigate a number of potential mechanisms linking the timing of electoral events with households' migration choices. After excluding that the evidence can be explained by mere political uncertainty, actual election results, fraudulent behavior on the part of incumbent governments aiming at maximizing pluralities, or technical reasons such as lags in voters' registration mechanisms, I test the hypothesis that the impact of the electoral schedule on the timing of household sorting is the consequence of a political budget cycle, i.e., fabrication of favorable economic conditions and implementation of generous spending and transfer policies right before the elections, followed by after-election austerity. To test this hypothesis, I employ a large panel dataset of yearly municipal budget and migration data spanning through two decades of Italian mayoral elections. The yearly panel data results reinforce the evidence of an election-driven cycle of sorting, with out-migration rates rising and in-migration rates falling sharply in the years following municipal elections. Moreover, I find that two key indicators of local government policy (the municipal budget surplus/deficit as a percentage of total current revenues and the local income tax rate) follow a time pattern that is compatible with a political budget cycle paradigm and exert a significant impact on out-migration and in-migration flows, thus pointing to the political budget cycle induced by the electoral schedule as the most likely structural cause of the observed time pattern of sorting.

The rest of the paper is structured as follows. Section **2** illustrates the institutional framework and the dataset of Italian municipalities. Section **3** analyzes the reduced-form impact of local election schedules on the timing of sorting, while section **4** tests a number of hypotheses about the mechanisms linking the timing of elections with the timing of sorting. Finally, section **5** concludes.

2 Institutional framework and dataset

The municipal level of government in Italy is made of over 8,000 authorities, with average population size of around 7,000 inhabitants and more than half the localities counting less than 3,000 residents. Irrespective of their size, all municipal authorities are statutorily responsible for the provision of services in two main areas.[3] The first area concerns environment-related services, and in-

[3] The sole exception is the possibility for small-sized municipalities to set up a flexible intermunicipal cooperation agreement or a formal institutional arrangement for the provision

cludes: public transportation systems; road maintenance, cleaning, and police; waste collection and management; water and sewer services; parks and green spaces; environmental monitoring, regulation and protection; planning and zoning policy (including the location of new productive plants); and management of infrastructures located within their boundaries. The second area concerns welfare-related services and includes: social care to elderly, physically disabled, drug and alcohol addicts, and mentally ill people (residential, community, and domiciliary care); services to families with dependent children (organization and management of pre-school/kindergarten services); help to lone-parent households, abused women, foreign immigrants and refugees (counselling, mediation, and advocacy); and financial assistance to homeless and indigent people (social housing; rent subsidies; food, health, and transport vouchers; unconditional one-off or periodic cash transfers).

While average annual per capita spending on municipal public services amounts to about euro 1,200, or almost 5% of GDP, the inter-municipal variance in the range and quality of services provided is enormous, particularly as far as welfare-related expenditures are concerned. In fact, EU, state and regional grants only cover about $\frac{1}{3}$ of spending on local welfare services, leaving municipal governments an ample degree of discretion. For instance, childcare services having a crucial role in female labour force participation according to the official Europe 2020 headline target strategies vary dramatically across municipalities, with a rate of formal enrollment of children aged 0-3 into nurseries ranging from less than 5% to over 50%, and municipal spending per child from less than euro 100 to over euro 3,000 (ISTAT, 2016). Similarly, while Italian municipalities spend on aggregate over half-billion euro per year on policies aimed at fighting poverty and social exclusion, less than half authorities offer a domiciliary social assistance service, and only about $\frac{1}{4}$ of them have a formal youth training and subsidised employment program (ANCI, 2016). Indeed, this suggests that the impact of municipal policies - in particular the structure, variety and intensity of welfare services - on the quality of life of households and on their location decisions can be substantial.

In fact, internal mobility has been a distinctive trait of contemporary Italian history. After a phase of soaring migration flows along the North-South gradient between the 1950s and 1970s due to the enormous economic gap between the

of public services that require a minimum scale of production (e.g., water and sewer services, or residential care homes). Services requiring a larger scale are provided by either of the two upper tiers of local government (120 Provinces and 20 Regions) or by the national government.

two areas of the country, the geographic pattern of migration has changed considerably in the most recent years: internal mobility fell between the early 1980s and the mid 1990s in spite of persisting interregional disparities and even rising unemployment rate differentials (Faini et al., 1997), to eventually increase again in a rather impressive way during the past two decades (Biagi et al., 2011). In 2014, total internal migration flows involved over 1.3 million people, an almost 20% larger figure than twenty years earlier, and long-distance or 'disequilibrium migration' flows from the poorer South to the more urbanised and developed Centre and North of Italy continue. Interestingly, though, the largest number of inter-regional moves in the most recent years have occurred within the North of Italy, and more than 60% of all internal migration episodes are represented by short distance within-province relocations (Buonomo and Gabrielli, 2016; Accetturo et al., 2019).

As for local elections, all Italian municipalities have direct election of the mayor every fifth year in a single or dual ballot depending on resident population size, with larger localities (>15,000 inhabitants) having a runoff stage among the two most voted candidates if none gets more than 50% of the votes in the first stage. The seats in the local council are assigned to the parties or lists supporting the mayoral candidates on a proportional basis, with a majority premium guaranteeing that the list supporting the elected mayor gets at least 60% of the council seats. Voting in mayoral elections is formally mandatory for all aged above 18 and average voter turnout is as high as 80%, though no sanctions exist for abstainers (Revelli, 2016).

The empirical analysis uses data on the about 7,000 localities that are situated in the fifteen "state law" continental Italy's regions (83 provinces) and thus exclude the five regions (the two islands *Sardegna* and *Sicilia*, and the three small alpine regions *Valle d'Aosta*, *Trentino-Alto-Adige,* and *Friuli-Venezia-Giulia*) that are entitled to larger autonomy ("home rule") and establish own limits and regulations on the municipal governments that are located within their boundaries, and where household mobility tends to be constrained by geographical and linguistic barriers. Table 1 reports the number of elections taking place during the period 2002-2014, showing that the largest fraction of municipalities voted in 2004 (June, 12), 2009 (June, 7), and 2014 (May, 26). Thanks to this overlapping schedule, I can identify the effect of election dates separately from common influences on migration propensities due, say, to national politics, demography, or macroeconomic conditions. Importantly, since the staggered structure of the schedule dates back several years or even decades (Repetto, 2018), I can take it as exogenous. In particular, I focus on the years 2004, 2009, and 2014, when elections were held in over 4,000 Italian municipalities (treated sample), using the almost 3,000 authorities not having elections in those years as the control group. Migration data limitations prevent me from using earlier years than the 2004 election wave.

I use municipal-level monthly population data so as to be able to observe the trajectory of migration through the months preceding and following the date of the election. All data used are official figures provided by the Italian National Statistics Institute (ISTAT). Using monthly migration data at the municipal level comes at the cost, though, of not having information on movers' destination: such information is available at the regional level only, thus allowing the construction of bilateral migration matrices at no lower than the inter-regional level. Basically, for each municipality and during every month I observe the total number of new registrations and the total number of cancellations from the municipal list of residents.[4] Monthly rates of out-migration and in-migration are around 0.3%, as shown in table 2.

Table 2 reports a further number of characteristics of the authorities in the treated and control samples, including the size of municipal population (observed on a monthly basis) and its age structure (observed on a yearly basis). First, the authorities holding elections in those three years tend to have smaller population and larger shares of mature residents than the authorities not holding elections. In order to account for these observable differences between the two groups, I report estimates of the effect of the electoral schedule on the timing of migration from specifications that use demographic characteristics as controls. A second feature of the data emerging from table 2 is that the number of authorities holding elections declines over time. Part of this phenomenon is due to episodes of redistricting and mergers of municipalities taking place between 2009 and 2014 and causing a fall in the total number of observations. Since those boundary changes can be considered as virtually mandated by the central government, they do not constitute a source of bias. For the most part, however, the decline is due to early termination of local legislatures leading to mayoral

[4] In addition, monthly migration data do not contain separate information on internal versus foreign migration flows (dalla Pellegrina et al., 2018). However, the annual municipal-level migration data that report this information show that foreign migration flows are of modest size relative to internal ones. I make use of annual data on internal migration flows in section **5** below.

elections before the expected end of the five-years term of office. About 300 of the 4,317 municipalities having elections in 2004 had the subsequent election before the end of the regular term (that is, between 2005 and 2008). Similarly, more than 300 of the 4,081 authorities holding elections in 2009 had an early election in the next four years. In general, an early election is required if the mayor dies, resigns, or is no longer backed by a majority of the council members. In addition, the Ministry of Interior can command anticipated elections if a council passes unlawful acts or excessive deficits. In order to rule out the possibility that our estimate of the effect of the timing of elections on the timing of sorting be driven by shocks affecting both the chance of holding an election in one of those years and resident households' willingness to move, Appendix **A** reports estimates on datasets that only include authorities holding elections after a regular five-years term of office.

3 The timing of elections and internal migration

I first investigate whether the schedule of mayoral elections has an influence on the timing of households' migration decisions by means of difference-in-differences estimation of a reduced-form migration equation on monthly panel data. The exogeneity of the staggered electoral calendar allows me to take the presence of a mayoral election in a locality in a given year as a quasi-randomly assigned treatment, and use the set of authorities not holding elections as the control group. Figures 1, 2, and 3 plot the migration rate trajectories in the vicinity of the election dates of 2004, 2009, and 2014. The upper portion of each figure reports the average monthly rates of migration (out-migration plus in-migration monthly flows as a percentage of resident population in a municipality on the first day of the month) in the group of localities holding an election (□) versus the average monthly rates of migration in the group of localities not holding an election (◯) in each of the January to May (pre-election) and June to October (post-election) months.[5] The lower portion of the figures shows the corresponding average monthly difference in migration rates between the two groups (◊), thus removing the seasonal aspects of migration that are apparent from the upper portion of the graph, such as the migratory peaks in early Spring and late Summer that seem likely to be attributable to retirees ('snowbirds')

[5] Appendix **A** reports empirical evidence based on narrower time windows around elections (up to two months), while section **4** performs a similar analysis on annual migration data over a long panel dataset (2002-2014).

and students moving for reasons that are unrelated to local elections.

The time pattern of migration that emerges from those figures is intriguing, and is remarkably consistent across the three election waves. First, migration rate trajectories in the treatment and control groups track each other closely in the pre-election months, offering no evidence of differential pre-treatment trends in the variable of interest depending on treatment status. However, they diverge visibly in the subsequent months, compatibly with the hypothesis of an effect of the presence of a mayoral election on the timing of migration in treated relative to control localities, with a particularly striking break in the spell immediately following (one to two months) the date of the election. As far as the 2004 and 2009 elections are concerned, the average growth in the rate of migration between the pre-election month (May) and the post-election month (June) in the treated authorities is almost twice as large as the one in the control authorities, and the difference gets even larger in the subsequent month. As for the 2014 election, the average growth of the migration rate from May 2014 to June 2014 in treated authorities (about 20%) is three times as large as in control group authorities (less than 7%), a gap that slightly decreases in the following months.

To formally test the impact of the timing of local elections on the trajectory of migration rates, I estimate a reduced-form migration equation for each of the three years (equation (1) below) that includes month dummies μ_t ($t \in T = \{\text{January}, ..., \text{October}\}$), a binary group membership indicator $e_i \in \{0, 1\}$ taking value 1 in all localities having an election in late May/early June of that year, and the interaction of the group indicator with a dummy variable equaling 1 for observations in the five months following the election (June to October):

$$m_{it} = \mu_t + \lambda e_i + \beta \left(e_i \cdot 1 \left[t \in \overline{T} \right] \right) + \theta_i + \varepsilon_{it} \tag{1}$$

where $\overline{T} = \{\text{June}, ..., \text{October}\}$. The panel structure of the data is captured by the time-invariant municipality-specific component θ_i, and ε_{it} is assumed i.i.d. The dependent variable m_{it} is measured as the sum of the inflow and outflow of individuals from municipality i's list of residents during month t, expressed as a percentage of resident municipal population on the 1^{st} day of the month.

I estimate equation (1) by OLS after taking deviations from municipal means, thus removing all time-invariant components. The estimation results are reported in table 3. The first line in table 3 shows baseline results of the specification in equation (1), while the second line includes the demographic size of localities as control. In all instances, the key coefficient β is estimated to be positive and statistically significant. Holding an election raises migration by up to 5% relative to the baseline average monthly migration rate.

To verify the robustness of this result, Appendix **A** first reports (table A.1) estimates of equation (1) on samples that only include authorities holding elections after a regular five-years term of office, with estimated βs that are virtually identical as the ones in table 3. Second, table A.2 in Appendix **A** reports the estimation results of equation (1) when employing time windows that are closer to the date of the election (8, 6, 4, and 2 months). The estimated coefficient on the post-election dummy β remains statistically significant across the different time spans in all years, averaging between 0.02 and 0.03, or an about 4% to 5% excess rate of migration in the post-election months in the authorities holding elections. As figures 1 to 3 suggest, the estimated β coefficient generally tends to increase in size and significance as the observation window narrows down. Finally, to rule out the possibility that the different trajectories in the treated and control groups be due in reality to underlying seasonal differences in migration patterns between the small and mostly rural localities in the treatment group and the larger and more urbanized ones in the control group, table A.3 reports analogous estimates as in table A.2 for the subgroup of localities having elections in 2003, 2008, and 2013, using the localities not having elections in those years (with the exclusion of the municipalities that constituted the treated group in 2004, 2009, and 2014) as the control group. The results in table A.3 using this alternative treatment group are remarkably similar to the ones in table A.2. The estimated coefficient on the post-election dummy is always positive, and it averages again around 0.3. As in the core sample, the β coefficient grows in size and significance as the observation window around the date of the election narrows down.

4 Discussion

The results in section **3** above point to a significant impact of the schedule of the elections on the timing of migration. However, such empirical evidence is in principle compatible with a number of distinct mechanisms linking electoral events with households' migration choices. Four such mechanisms are discussed in what follows. The first mechanism that might be responsible for the delay of migration to after the election is political uncertainty, in particular the idea that new information on the local public goods that will be provided and on the taxes that will be imposed in the future can be acquired by waiting and procrastinating the migration decision until after the elections, when uncertainty will be resolved. The second is related to the possibility that the actual electoral outcome rather than electoral uncertainty per se be responsible for post-electoral migration flows, that would therefore crucially depend on whether the incumbent gains re-election or not. The third has to do with candidates' efforts at maximizing votes, and in particular with their attempts at mobilizing non-resident supporters to fictitiously relocate in order to register as eligible voters where elections are held. The fourth and final mechanism that I consider refers to the opportunistic behavior of incumbent governments engineering a political budget cycle of pre-electoral expansionary fiscal policy and post-electoral austerity.

4.1 Electoral uncertainty

Recent empirical research has found a number of dynamic business decisions to be systematically influenced by the timing of elections irrespective of purposeful policy, or even in the opposite direction as what traditional political business cycle models of opportunistic policy-making would predict (Canes-Wrone and Park, 2012; Baker et al., 2016; Colak et al., 2017; Jens, 2017). In the presence of political uncertainty induced by competitive elections (regarding, for instance, what kind of taxation, privatization, and labor-market regulation policies will be implemented by the newly elected government), domestic corporate capital expenditures and foreign direct investments across a large number of countries facing parliamentary or presidential elections appear to drop significantly before the election and stagnate until after it, when political uncertainty is resolved (Julio and Yook, 2012; 2016).[6] Such incentives for procrastination of large investments with high costs of reversal (*e.g.*, building a new plant) would partly offset any concomitant attempt of opportunistic election-year manipulation of public budgets, or even generate a 'reverse electoral business cycle' of pre-electoral stagnation, and would explain the limited empirical support for the conventional political business cycle hypothesis (Canes-Wrone and Park, 2012).

Similarly, one could see the migration decision as an investment: the decision

[6] The theoretical foundations of this line of investigation can be traced back to the models of irreversible investment of Cukierman (1980), Bernanke (1983), and Rodrik (1991). Following a similar approach, Giavazzi and McMahon (2012) document effects of electoral uncertainty on households' precautionary savings.

to move involves up-front fixed costs that are to some extent irreversible, it is taken in an uncertain environment where new information can be acquired by waiting, and it is postponable, in the sense that the potential migrant can either keep the valuable option to procrastinate, or kill it by exercising it (Burda, 1995). The fact that new information on the local public goods that will be provided in the future can be acquired by waiting and delaying the migration decision until after the elections, when uncertainty will be resolved, would give rise to a voting and sorting cycle, with residents that are in principle willing to move in search of the public services they require (e.g., schooling for their children, or care homes for their elderly relatives) delaying their *exit* decision (Hirschman, 1970) to after the election.

Testing this hypothesis requires a proxy of the degree of uncertainty about the policy that will be enacted by the new government in the next term. In the absence of ideal ex ante measures of uncertainty that are continuously registered during the term leading to the election, I rely on indicators that have been widely employed in the literature and are computed on: a) ex post data from the election of interest; b) data from previous elections in the same municipality; c) data from elections that have taken place during the previous term of office for overlapping tiers of government (Fiva and Natvik, 2013).

First, I employ the win margin of the mayor in the coming election, i.e., the difference in votes between the elected mayor and its most voted opponent, as an ex post indicator of closeness of the race. To have comparable figures across municipalities, the win margin is standardised by expressing the absolute difference in votes as a percentage of the total votes cast. This way, the closeness of the race index lies between 0 and 100.[7] In the 2004 election wave, 4,006 races were contested by at least two candidates, and the mean and median win margins were 20.8% and 16.9%. In 2009, with 3,770 contested elections, mean and median win margins were 20.2% and 16% respectively. Finally, in the 3,201 contested elections held in 2014, the mean and median win margins were 23.1% and 16.6% of the votes. Overall, the win margin was less than 3 percentage points in 10% of the elections, it was less than $1\frac{1}{2}$ percentage points in 5% of the elections, and it was less than 0.3 percentage points in about 1% of the elections. Almost 30 races in those three election waves ended in exact ties and required supplementary rounds.

[7] The first round outcome is considered in the few cases where the election has a run-off stage.

Table 4 reports the estimation results of a migration equation that includes the above indicators of electoral uncertainty in each of the three election waves. The first three columns use dummy variables that equal 1 if the vote margin between the elected mayor and the most voted opponent is less than 0.3 percentage points, $1\frac{1}{2}$ percentage points, and 3 percentage points, interacting them with the after-election dummy. In all cases, the post-election dummy for the authorities actually voting remains of the same size as before and statistically significant, pointing to an excess rate of post-election out-migration from municipalities facing an election at the midst of the period relative to municipalities where no election takes place of about five percent. However, the estimated coefficients on the interaction terms are not statistically significant, thus offering no evidence of an impact of electoral uncertainty on the timing of migration besides the presence of a mayoral election per se.

Second, given that ex post indicators suffer from an endogeneity problem due to the fact that the ex ante and usually unobserved degree of uncertainty affects voter turnout and the latter affects the outcome of the election (Shachar and Nalebuff, 1999), I use proxies of uncertainty from previous electoral rounds where available (elections of 2009 and 2014). In particular, the fourth column of table 4 employs a binary indicator based on the rate of voter turnout in the previous mayoral election (2004 and 2009 respectively) as a predictor of the degree of uncertainty in the coming election. In this case too, the indicator taking value 1 if turnout exceeded 80% in the previous election (roughly corresponding to the 10^{th} percentile of the turnout distribution) is not estimated to affect the timing of migration decisions beyond the sheer presence of an election in a municipality. Similar (unreported) results emerge when using other uncertainty indicators related to outcomes or voter participation in the previous elections.

Finally, the last column of table 4 reports the estimation results when using a proxy of uncertainty from the Senate elections that were held in Italy the year before the mayoral elections of 2009 and 2014. In particular, I build a dummy variable that equals 1 if the vote margin between the candidate that is elected Senator and its opponent in a winner-takes-all race at the provincial level is less than 1.5 percentage points.[8] However, estimation of this specification (as well as specifications based on other proxies of electoral uncertainty from the Senate

[8] Admittedly, the closeness of the Senate race might not be an appropriate predictor of the closeness of a mayoral race in this context given that races for the Italian Senate tend to have a partisan dimension (typically, a competition between candidates from the national right-wing and left-wing parties) that is less explicit or common in municipal elections.

elections) fails to return any significant impact of uncertainty on the timing of migration.

Overall, the results in table 4 confirm that the schedule of elections affects the timing of migration significantly, but they provide no evidence of a specific influence from the indicators of electoral uncertainty that we have employed. In fact, the post-election rise in migration rates occurs irrespective of the degree of uncertainty that characterises the election, at least as we have been able to proxy it. One possible explanation for this result is that the out-migration and in-migration flows that I have treated jointly in a single variable respond in a different way to the incentives generated by a jurisdiction's political climate. In particular, out-migrants might have better information about the candidates and the degree of uncertainty of the next mayoral race in their jurisdiction of residence than potential in-migrants. To see whether this is the case, figures 4 to 9 depict the distinct trajectories of out-migration and in-migration rates in the vicinity (10-months window) of the three election dates. Similarly to figures 1 to 3, the upper portion of each figure reports the average monthly rates of out-migration/in-migration (monthly population outflows/inflows as a percentage of resident population in a municipality on the first day of the month) in the group of localities holding an election (□) versus the average monthly rates in the group of localities not holding elections (◯) in each of the January to May (pre-election) and June to October (post-election) months. The lower portion of the figures shows again the corresponding average monthly difference in out-migration rates (figures 4-6) and in-migration rates (figures 7-9) between treatment and control groups (◊).[9]

Consider the path of out-migration in figures 4-6 first: similarly to overall rates of migration, monthly out-migration rates in the treatment and control groups almost overlap in the pre-election months. However, their trajectories are considerably different after the date of the election, with treatment group localities' figures significantly exceeding control group localities' ones. Figures 4 to 6 reveal in particular that the growth in out-migration rates between the pre-election and post-election months in the treatment group is far larger than in the control group (from over twice as much in 2004 and 2009 to almost four

[9]Figures 7 to 9 show that the average rates of in-migration in the localities holding an election tend to be systematically higher than in control localities. This is due to fact that the localities holding elections tend to be smaller (table 2) and are located in the regions of the North of Italy, and those regions are net recipients of migrants.

times larger in 2014). On the other hand, the picture emerging from figures 7 to 9 referring to in-migration rates is altogether different. There seems to be no evidence of immigrants postponing their moving decisions to after the elections in the group of authorities actually facing them. Rather, the average difference in in-migration rates between the treatment and control samples shows a tendency to decline in the post-election months.

Table 5 reports the estimation results of equation (1) when using out-migration and in-migration rates as the dependent variable in each of the three election years. As far as the out-migration equation is concerned, it turns out that authorities facing a municipal election towards late May/early June register significantly higher rates of out-migration in the months following the election. On average, and given monthly rates of out-migration of about 0.3%, the estimated coefficients in the first row of table 5 suggest that holding an election provokes an increase in out-migration during the post-election months of almost 10%, with the estimate slightly declining over time. Conversely, no such effect emerges from the second row of table 5 as far as in-migration rates are concerned. The impact of the election even turns out to be negative and statistically significant at the 5% and 10% level in the years 2009 and 2014 respectively. Finally, in order to ascertain whether uncertainty has a differential impact on out-migration versus in-migration flows, I employ all indicators of uncertainty that I have used above in the distinct equations for out-migration and in-migration rates. As far as these electoral uncertainty proxies are concerned, though, tables 6 and 7 show that they do not play any role in either.

4.2 Election outcomes

An alternative mechanism that could explain the role of elections in migration decisions refers to the actual changes of government that occur at elections. In particular, the dependence of the timing of internal migration flows on the electoral schedule might originate in environments of politically unstable jurisdictions that are stuck in inferior configurations in terms of equilibrium policy choices and allocation of citizens to localities, and where the ousting of the incumbent in one of them constitutes the shock that is transmitted to the system and allows it to break out through citizens re-sorting and eventually attaining a higher level of aggregate welfare (Kollman et al., 1997).

An ideal test of this hypothesis requires well-defined partisan races in unstable political environments, where the periodic occurrence of an incumbent losing to an opponent of a different ideology is followed by a policy reversal that can credibly bring about a new allocation of households to communities after the election. In the context we are dealing with here, the fact that incumbent mayors are labelled as non-partisan in about $\frac{2}{3}$ of the elections (though most of the times they are de facto linked to a political party, union, or interest group) makes a test of this hypothesis admittedly problematic. More generally, though, the trajectories of out-migration and in-migration rates that we observe in the vicinity of elections seem virtually orthogonal to actual electoral outcomes: when building a dummy variable that equals 1 at an election where the incumbent mayor loses to an opponent that is supported by a coalition of a different nature - i.e., a left-wing mayor losing to a right-wing or to a non-partisan candidate, or vice versa, a category of events occurring in less than 20% of all elections of 2009 and 2014 - and interacting that dummy variable with the usual election month indicator, I get no evidence that the size of the migration flows occurring during the months following the date of the election depends on whether the incumbent gained re-election or not, thus casting doubts on the relevance of a political-instability mechanism in explaining the impact of the electoral schedule on the timing of internal migration decisions.

4.3 Fictitious relocations

Third, it has been suggested in other contexts that incumbents might purposefully alter the number of resident voters right before the date of the election in order to enhance their chances of re-election. This could occur, for instance, if mayors could convince partisan voters residing elsewhere to register for voting in jurisdictions having a close election to tilt it in their favour. In this regard, Fukumoto and Horiuchi (2011) test whether the combination of weak registration requirements for voting in Japanese municipal elections (simply submitting a form, even through a delegate) and an asymmetric electoral schedule might induce candidates to mobilise non-resident supporters to fictitiously register there. They adopt an "election timing as treatment" approach similar to the one that I employ here, and use municipality-month panel data around the April 2003 municipal election round in Japan to find that the treatment (having an election) has a significant impact on the number of relocations taking place before the election in a locality. To be fair, that mechanism seems unlikely to be at work in the context we analyze here. Electoral registration requirements in Italy are rather strict: registering to vote in an Italian municipality involves starting in person a formal and rather time-consuming administrative process of change of residence. Importantly, proofs of actual relocation must be provided, and are subject to local police review. Since such strategy is costly, we would expect to see it in place only where uncertainty plays a major role and the incumbent runs the risk of being unseated, something that the evidence above allows us to rule out.

A potentially relevant related issue is that the lists of residents might be updated with a lag for technical reasons that have little to do with the actual timing of households' residential choices. However, any change of residence that is initiated by an Italian citizen in a given municipality by submission of a formal request implies immediate cancellation from the list of residents of the community of origin and the simultaneous new registration in the municipality of destination. This means that a request of change of residence becomes immediately effective for all purposes (most importantly for what concerns local tax dues or rights to, say, domiciliary assistance or housing benefits), with the possibility on the part of the municipal government of destination to declare the act null and start a prosecution if the relocation turns out to be fictitious (in fact, fictitious relocations tend to work as a means of tax avoidance rather than as an attempt at influencing the outcome of an election). In addition, if a technical lag issue was really at work, it would indeed affect both in-migration and out-migration flows, and in the same direction.

4.4 The political budget cycle

Finally, one might wonder whether the influence of the electoral schedule on the time pattern of internal migration is due in reality to the fact that municipal policies are manoeuvred strategically by opportunistic incumbent governments and tend to follow a political budget cycle, with spending and budget deficits rising and taxes falling right before the elections, and restrictive fiscal austerity measures being postponed to after the elections. This implies, first, that potential out-migrants might find it profitable to postpone *exit* to after the elections that are scheduled in their municipality of residence in order to fully take advantage of the generous pre-electoral policies and avoid paying their deferred price (higher local taxes and lower levels of public services in the aftermath of the elections). At the same time, the restrictive policies that governments need to implement at the start of their term of office would create the least favorable conditions for *entry* after an election.

In fact, and in addition to the literature reviewed in the introduction documenting the widespread opportunistic use of fiscal policy by local governments around the world, recent empirical contributions by Alesina and Paradisi (2017), Bonfatti and Forni (2017) and Repetto (2018) show that Italian municipal governments strategically manoeuvre policy tools. Alesina and Paradisi (2017) exploit the introduction of a new municipal property tax in the early 2010s and the staggered electoral schedule to demonstrate that local governments impose lower tax rates if they face an election in the subsequent year. Bonfatti and Forni (2017) document that Italian municipal governments set higher levels of public spending, particularly as far as investment spending is concerned, and provoke a general deterioration of public budgets when they make their fiscal decisions close to the next elections. Repetto (2018) reports evidence of electoral cycles in municipal investments on roads, parks and public housing, and finds that dissemination of financial information has the effect of smoothing those cycles.

Given that the existing budget-making constraints on Italian local governments require their key fiscal policies to be set before December 31^{st} of each year and to hold for the subsequent calendar year, I employ here an yearly panel dataset to test the hypothesis that the electoral calendar influences the timing of out-migration and in-migration flows through changes in municipal fiscal policy. The panel dataset includes the same 7,000 continental Italy's municipalities studied above, but it spans through over a decade (years 2002-2014), for a total of almost 90,000 annual observations on internal migration flows and municipal fiscal policy instruments. The time period considered here saw the Italian economy move from moderate economic growth in the early 2000s to a deep and prolonged recession during the late 2000s and the early 2010s, and the average annual internal migration rates show a moderate increase over the decade, from below 2.5% to close to 3%.

First, in order to verify if the pattern of election-driven migration that was observed on monthly data emerges on annual data too, table 8 reports the results of estimation of reduced-form out-migration and in-migration equations (equation (2) below) in a similar spirit as the monthly-level equation (1). Given the five-years electoral term, the municipalities in this dataset face two to three elections during the period of observation (table 1), and equation (2) lets the yearly percentage rate of out-migration (subsequently, in-migration) from (to) locality i to (from) any other Italian locality in year t (M_{it}) be a function of the distance of year t from the year of the next election in locality i (d_{it}):

$$M_{it} = \phi(d_{it}) + h_i + y_t + \xi_{it} \tag{2}$$

I start with a linear measure of distance from elections, $\phi(d_{it}) = \delta\left[\frac{1}{4}(\tau_{it} - t)\right]$, with τ_{it} as the first election year in municipality i following year t, and normalization of the measure having the only scope of easing interpretation of the coefficient (Berdejó and Yuchtman, 2013). This formulation makes the distance from the next election range from 0 in the year of the elections to 1 in the first year of a regular five-years term, and imposes the effect of election distance on out-migration (in-migration) to be captured by a single parameter (δ). Next, I model the effect of the timing of municipal elections on the timing of migration in a more flexible nonlinear way through a vector of four dummy variables $e(t-l)_{it}$ that equal 1 if there was an election in the $l = 1, ..., 4$ calendar years preceding the migration measurement year t in municipality i:

$$\phi(d_{it}) = \sum_{l=1}^{4} \delta_l e(t-l)_{it} \tag{3}$$

A problem that might be caused by the use of annual data at the municipal level is the influence on migration rates of correlated macroeconomic conditions across the country and their cyclical fluctuations. To account for such macroeconomic conditions potentially affecting all municipalities in a similar way, equation (2) includes fixed year effects (y_t) that can be identified separately from the effects of the elections (parameters δ_l) thanks to the staggered nature of the electoral schedule. Finally, I control for locality-specific unobserved time-invariant traits via municipal fixed effects (h_i), while ξ_{it} is an i.i.d error term.

I report the within-group estimates of the reduced-form models for out-migration and in-migration rates in table 8. The first column reports estimates of the δ coefficient on the linear measure of distance. As for out-migration, the estimated coefficient is an highly significant +0.047 (standard error = 0.011). Given an average annual out-migration rate of about 2.7%, this means that the rate of out-migration during the year immediately following an election year is almost 2% higher than at the end of the electoral term. As for in-migration, the estimated δ coefficient is negative (meaning that moving from the beginning of an election cycle to the end of term one can expect in-migration to rise), somewhat larger in absolute value (−0.060), and highly statistically significant (standard error = 0.014). In relative terms, the effects are smaller than the ones obtained from the finer observations on monthly data, probably due to the fact that using annual observations implies renouncing to the action taking place within a year in the vicinity of the date of the election.

The next column of table 8 reports the estimation results from a specification having a one-year lagged election dummy - $e(t-1)_{it} = 1$ if the migration rate is measured the year after a municipal election in municipality i is held, 0 in any other year of the electoral cycle - while the subsequent columns keep on adding further lagged election year dummies for up to an election having taken place four years back. In all of those specifications, it turns out that the timing of elections significantly influences the trajectory of migration out of a municipality. In particular, out-migration is estimated to be higher in the year following an election and, to an even larger extent, in the subsequent year. The estimates including dummies for three and four years after an election reveal that the rate of out-migration reverts to the average in the third year, and turns significantly negative in the fourth year, that is the year corresponding to the eve of the next election in a regular five-years term of office. Similarly to the linear distance model, these estimates suggest that the out-migration rate is almost two percent larger in the post-election year than in the pre-election one.

The estimation results for the in-migration equation are reported in the lower panel of table 8, and point to a clear election-driven pattern too. However, similarly to the results obtained when using a linear distance measure, in-migration rates appear to respond to the calendar of municipal elections in the opposite way as out-migration rates. The smallest of all rates of entry that are registered during an electoral cycle is observed in the post-election year. In the subsequent years, the in-migration rate keeps on increasing and reaches its maximum in the election year, when it is over 3% larger than in the post-election year. Figure 10 depicts the out-migration and in-migration trajectories between two elections that the estimation results from the last column of table 8 (full set of distance-from-election dummies) suggest to be typical.

Overall, the reduced-form estimates based on this large yearly panel dataset seem compatible with a political budget cycle of incumbent governments creating a favorable 'fiscal climate' before the elections and postponing restrictive policies that discourage in-migration and favor out-migration to after the elections. In order to ascertain the plausibility of this interpretation of the observed migration patterns, I employ two indicators of local fiscal policy that are available for the period 2006-2014 and can be viewed as fundamental signals of local governments' budgetary choices having an impact on households' welfare. The first is the yearly budget surplus (or deficit) as a percentage of total current municipal revenues. After adhering to the EU Stability and Growth Pact (1997), Italy adopted a set of internal fiscal rules (known as the Domestic Stability Pact) that were aimed at extending fiscal discipline to all subnational levels of government in order to ease coordination and attain the EU-wide consolidated fiscal targets. The rules restraining local government deficit spending are approved yearly by the Italian Parliament and have shown considerable volatility over time, from limits on issuance of new debt to caps on expenditures and explicit targets on local fiscal gaps (municipal spending net of interest outlays minus own revenues), generating some confusion and opacity in central-local fiscal relationships. To enforce these rules, central government imposes penalties in terms of grant cuts and hiring limits on non-complying authorities (Grembi et al., 2016). Basically, deviations from the balanced budget rule can only be temporary, and fiscally distressed authorities need to commit to a budget consolidation plan. In the panel dataset that we employ here, the median budget surplus is 0.12%, corresponding to a virtually balanced budget, but its variance is considerable, with the largest deficit amounting to almost -10% and the largest surplus to over +20%.[10]

The second indicator I use is the income surcharge rate that Italian municipalities can impose since the year 2000 on personal income on top of the progressive tax rate structure set by the national government. While the impact of income taxes on taxpayers' location decisions is well documented in the literature as far as federal countries are concerned (Schmidheiny, 2006), the fiscal decentralization wave of the recent decades has contributed to make it a salient issue in unitary countries too (Agrawal and Foremny, 2019). At the time of its introduction, the municipal surcharge was a flat rate of up to 0.8% to be applied on personal income. Starting from 2011, local authorities have been given the chance to establish a progressive income surcharge too within the five income tax brackets set by central government. About $\frac{1}{3}$ of municipal authorities switched from a proportional to a progressive tax structure in the subsequent years, with marginal tax rates ranging from around 0.4% in the lowest to 0.9% in the highest tax bracket. While the ensuing interjurisdictional tax

[10] Local government budgets are available for the years 2006-2014 (ISTAT, Indicatori di bilancio e del personale).

rate differentials are not overwhelming, they tend to be perceived as signals of the more general municipal fiscal strategy and might therefore have an impact on taxpayers' behavior and location decisions.

Figure 11 reports the trajectories of these two fiscal variables between subsequent elections. In particular, for the income tax I use the municipal marginal tax rate on mean per capita income (euro 20,000). Each $\Diamond$ represents the point estimate (along with its 95% confidence interval) of the coefficient on the lagged election dummies (δ_l) in equations (2)-(3), where the budget surplus as a percentage of current revenues and the local income tax rate are employed as dependent variables respectively. The estimated δ_l coefficients in figure 11 point to time patterns of those policy variables that are compatible with a political budget cycle paradigm. The budget surplus significantly improves in the years following an election year. Afterwards, it starts progressively deteriorating until the next election. Similarly, the local income tax rate sharply increases in the year following an election year, and then falls during the entire term of office, touching its minimum level in the year the next election is held. I employ these two variables to perform a direct test of the impact of local fiscal policy on the trajectories of out-migration and in-migration rates, and exploit once more the exogeneity of the staggered electoral schedule by using the set of distance-to-election dummy variables $e(t-l)_{it}$ as instruments. The instrumental variables estimation results of this model are reported in table 9, and point to a significant impact of those fiscal variables on interjurisdictional sorting flows. Out-migration from a locality rises and in-migration into a locality falls significantly when the government of that locality puts restrictive fiscal policies (larger budget surpluses and higher local income tax rates) in place. The Sargan test on the instruments fails to reject the null of instrument validity in both equations. Of course, I cannot exclude that part of the effect that I have estimated be due to the fact that the two variables that I have used are correlated with aspects of local policy-making that cannot be observed and have an impact on households' location decisions by contributing to the fiscal climate of a locality, such as the quality of public services or the employment opportunities in the local public sector.

5 Concluding remarks

This paper has designed and run a test of Tiebout sorting that relies on the staggered time structure of local elections as a recurrent exogenous perturbation to the equilibrium allocation of households to communities in a system of local governments. I have first employed monthly data on municipal-level migration rates around the three largest waves of Italian local elections of the past two decades (June 12, 2004, June 7, 2009, and May 26, 2014) to test the hypothesis that the timing of elections has an influence on the timing of migration decisions. The empirical analysis employs a difference-in-differences estimation approach using the fact that around 4,000 municipalities are treated, in the sense that they hold elections in those three years, while the about 3,000 municipalities not holding elections in those years due to the staggered election schedule act as the control group. Average migration rates in the months following the date of the election are estimated to be up to five percent higher than the respective figures in the months preceding the date of the election in the localities that actually hold elections. If we take this figure as an estimate of the actual fraction of the moving population that does so in a Tiebout-like fashion, it is of the same order of magnitude as typical estimates from countries like the US (Banzhaf and Walsh, 2008), where household mobility is usually believed to be significantly larger (Molloy et al., 2011).

Next, I have explored a number of mechanisms that could potentially be responsible for the impact of the electoral calendar on the timing of internal migration decisioins. I find a number of proxies of electoral uncertainty to have insignificant effects on the timing of migration decisions, and inspection of the distinct trajectories of out-migration and in-migration rates reveals that the former rise and the latter tend to fall after mayoral elections, and in neither case are either electoral uncertainty or actual election results estimated to play a role. After excluding that the impact of the timing of elections on sorting can be explained by technical reasons (mechanical delays in registration) or fraudulent behavior (fictitious relocation), I test the hypothesis that opportunistic budget cycles of pre-electoral fiscal expansion and post-electoral austerity be responsible for the observed migration trajectories. I find evidence that two key indicators of local government policy (the municipal budget surplus/deficit as a percentage of total current revenues and the local income tax rate) exhibit a time pattern that is compatible with a political budget cycle (larger deficits and lower tax rates before the elections, followed by fiscal consolidation after the elections), and that they exert a significant impact on out-migration and in-migration flows in a structural form model that uses the exogenous dates of staggered municipal elections as instruments.

Overall, both the evidence based on monthly data around the three large election waves of 2004, 2009, and 2014, and the one on the yearly panel dataset point to the influence of the electoral calendar on the timing of intermunicipal sorting decisions, and to its usefulness in identifying Tiebout-like incentives in migration decisions separately from conventional push and pull factors that are unrelated to the demand for local public goods over which localities vote periodically. In particular, the panel data evidence on the opportunistic use of fiscal policy instruments by incumbents and their impact on moving decisions suggests that the political budget cycle induced by the electoral schedule be the most likely cause of the observed time pattern of sorting.

Indeed, the migration decision within a decentralised structure of government is the product of a large number of considerations (Blomquist et al., 1988; Day and Winer, 2006), and Tiebout-like incentives are likely to play different roles in long versus short-distance movements (Biagi et al., 2011) or in temporary versus permanent migration (Fuchs-Schündeln and Schündeln, 2009). The fact that neither of these issues can be addressed with the data that I have employed here calls for future investigations of the impact of recurrent local elections on migration based on micro-data. In addition, the evidence presented in this paper arguably raises further potentially important issues for theoretical and empirical research. An issue that seems worth of consideration concerns the analysis of how distinct aspects of local public expenditures - *e.g.*, the generosity of housing subsidies, unemployment benefits, and childcare services, versus domiciliary assistance and health care programs - affect the location choices of groups of residents that may be characterised by different degrees of mobility. This seems an important issue to address when studying how electoral incentives shape the making of local policy and the resulting socio-economic and demographic complexion of localities in a decentralised welfare provision structure.

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Tables

Table 1 Schedule of Italian municipal elections

year	
2002	733
2003	321
2004	4,317
2005	367
2006	1,161
2007	773
2008	425
2009	4,081
2010	461
2011	1,176
2012	777
2013	528
2014	3,677

Notes: Data from 6,702 municipalities located in the 15 regions of continental Italy. Source: Ministero dell'Interno, Governo Italiano (www.interno.gov.it).

Table 2 Italian municipal elections: 2004, 2009, 2014

	2004 election		2009 election		2014 election	
	yes	no	yes	no	yes	no
municipalities	4,317	2,384	4,081	2,620	3,677	2,958
monthly out-migration rate (%)	0.26	0.23	0.26	0.23	0.28	0.26
monthly in-migration rate (%)	0.36	0.29	0.31	0.27	0.32	0.31
population	4,824	11,927	5,206	11,401	4,837	10,389
age $<$ 18 (%)	15.48	17.08	15.54	16.27	15.48	15.86
age 18-25 (%)	8.33	9.23	7.64	8.48	7.40	8.17
age 26-35 (%)	14.63	14.74	13.31	13.51	11.31	11.84
age 36-45 (%)	15.09	14.94	15.99	15.60	15.33	15.04
age 46-55 (%)	13.29	12.96	13.61	13.47	15.11	14.95
age 56-65 (%)	12.02	11.39	12.18	11.76	13.08	12.75
age $>$ 65 (%)	21.16	19.66	21.72	20.91	22.29	21.39

Notes: Source: ISTAT, Istituto Nazionale di Statistica (www.istat.it).

Table 3 Monthly migration equation (1)

	2004	2009	2014
$\widehat{\beta}$	0.0298***	0.0179***	0.0133**
	(0.0060)	(0.0055)	(0.0052)
$\widehat{\beta}^{(a)}$	0.0279***	0.0171***	0.0132**
	(0.0060)	(0.0055)	(0.0052)
obs.	67,010	67,020	66,350

Notes: Panel data (within-groups) estimates of parameter β from equation (1); months=January to October. $^{(a)}$: equation (1) includes the size of municipal population on the first day of each month among the regressors. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table 4 Monthly migration equation (1): electoral uncertainty

			2004		
$\widehat{\beta}$	0.0299***	0.0294***	0.0310***		
	(0.0060)	(0.0061)	(0.0062)		
uncertainty(I)	-0.0142				
	(0.0362)				
uncertainty(II)		0.0061			
		(0.0174)			
uncertainty(III)			-0.0126		
			(0.0128)		
			2009		
$\widehat{\beta}$	0.0179***	0.0183***	0.0195***	0.0193***	0.0177***
	(0.0055)	(0.0057)	(0.0055)	(0.0059)	(0.0055)
uncertainty(I)	-0.0040				
	(0.0339)				
uncertainty(II)		-0.0083			
		(0.0279)			
uncertainty(III)			-0.0176		
			(0.0168)		
uncertainty(IV)				-0.0041	
				(0.0087)	
uncertainty(V)					0.0087
					(0.0275)
			2014		
$\widehat{\beta}$	0.0135**	0.0133**	0.0136**	0.0119**	0.0135**
	(0.0052)	(0.0053)	(0.0054)	(0.0053)	(0.0053)
uncertainty(I)	-0.0315				
	(0.0432)				
uncertainty(II)		0.0002			
		(0.0152)			
uncertainty(III)			-0.0032		
			(0.0126)		
uncertainty(IV)				0.0069	
				(0.0111)	
uncertainty(V)					-0.0034
					(0.0136)

Notes: dependent variable = monthly migration rate (%); post-election dummy variable is interacted with the following indicators: uncertainty(I): win margin in the upcoming election is less than 0.3%; uncertainty(II): win margin in the upcoming election is less than 1.5%; uncertainty(III): win margin in the upcoming election is less than 3%; uncertainty(IV): turnout in previous election > 80%; uncertainty(V): win margin in previous Senate election (2008; 2013) is less than 1.5%. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table 5 Monthly out-migration and in-migration (equation (1))

	2004	2009	2014
		out-migration	
$\widehat{\beta}$	0.0286***	0.0266***	0.0201***
	(0.0039)	(0.0036)	(0.0036)
		in-migration	
$\widehat{\beta}$	0.0012	-0.0088**	-0.0068*
	(0.0047)	(0.0044)	(0.0038)
obs.	67,010	67,020	66,350

Notes: Panel data (within-groups) estimates of parameter β from equation (1); months=January to October. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table 6 Monthly out-migration equation & uncertainty

			2004		
$\widehat{\beta}$	0.0284***	0.0281***	0.0284***		
	(0.0039)	(0.0039)	(0.0040)		
uncertainty(I)	0.0103				
	(0.0255)				
uncertainty(II)		0.0087			
		(0.0092)			
uncertainty(III)			0.0019		
			(0.0072)		
			2009		
$\widehat{\beta}$	0.0267***	0.0264***	0.0267***	0.0259***	0.0263***
	(0.0036)	(0.0036)	(0.0036)	(0.0039)	(0.0036)
uncertainty(I)	-0.0066				
	(0.0231)				
uncertainty(II)		0.0056			
		(0.0128)			
uncertainty(III)			-0.0002		
			(0.0083)		
uncertainty(IV)				0.0022	
				(0.0053)	
uncertain(V)					0.0149
					(0.0177)
			2014		
$\widehat{\beta}$	0.0201***	0.0197***	0.0200***	0.0202***	0.0205***
	(0.0036)	(0.0036)	(0.0037)	(0.0037)	(0.0036)
uncertain(I)	0.0021				
	(0.0261)				
uncertain(II)		0.0080			
		(0.0091)			
uncertain(III)			0.0013		
			(0.0068)		
uncertain(IV)				-0.0002	
				(0.0076)	
uncertain(V)					-0.0049
					(0.0117)

Notes: dependent variable = monthly out-migration rate (%); post-election dummy variable is interacted with the following indicators: uncertainty(I): win margin in the upcoming election is less than 0.3%; uncertainty(II): win margin in the upcoming election is less than 1.5%; uncertainty(III): win margin in the upcoming election is less than 3%; uncertainty(IV): turnout in previous election > 80%; uncertainty(V): win margin in previous Senate election (2008; 2013) is less than 1.5%. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table 7 Monthly in-migration equation & uncertainty

			2004		
$\widehat{\beta}$	0.0015	0.0014	0.0026		
	(0.0048)	(0.0048)	(0.0049)		
uncertainty(I)	-0.0246				
	(0.0214)				
uncertainty(II)		-0.0026			
		(0.0135)			
uncertainty(III)			-0.0144		
			(0.0097)		
			2009		
$\widehat{\beta}$	-0.0088**	-0.0081*	-0.0071	-0.0065	-0.0086*
	(0.0044)	(0.0042)	(0.0042)	(0.0045)	(0.0045)
uncertainty(I)	0.0025				
	(0.0236)				
uncertainty(II)		-0.0139			
		(0.0355)			
uncertainty(III)			-0.0174		
			(0.0196)		
uncertainty(IV)				-0.0063	
				(0.0078)	
uncertain(V)					-0.0062
					(0.0184)
			2014		
$\widehat{\beta}$	-0.0065	-0.0064	-0.0064	-0.0083**	-0.0069*
	(0.0038)	(0.0039)	(0.0040)	(0.0039)	(0.0039)
uncertainty(I)	-0.0337				
	(0.0465)				
uncertainty(II)		-0.0078			
		(0.0132)			
uncertainty(III)			-0.0045		
			(0.0085)		
uncertainty(IV)				0.0072	
				(0.0086)	
uncertainty(V)					0.0015
					(0.0101)

Notes: dependent variable = monthly in-migration rate (%); post-election dummy variable is interacted with the following indicators: uncertainty(I): win margin in the upcoming election is less than 0.3%; uncertainty(II): win margin in the upcoming election is less than 1.5%; uncertainty(III): win margin in the upcoming election is less than 3%; uncertainty(IV): turnout in previous election > 80%; uncertainty(V): win margin in previous Senate election (2008; 2013) is less than 1.5%. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table 8 Elections & migration: yearly panel data

	out-migration				
linear distance	0.0468***				
	(0.0114)				
election(t-1)		0.0321***	0.0365***	0.0373***	0.0177*
		(0.0076)	(0.0082)	(0.0089)	(0.0109)
election(t-2)			0.0429***	0.0444***	0.0317***
			(0.0075)	(0.0084)	(0.0100)
election(t-3)				0.0095	-0.0025
				(0.0087)	(0.0103)
election(t-4)					-0.0287***
					(0.0103)
	in-migration				
linear distance	-0.0596***				
	(0.0146)				
election(t-1)		-0.0687***	-0.0720***	-0.0721***	-0.0985***
		(0.0092)	(0.0100)	(0.0115)	(0.0135)
election(t-2)			-0.0385***	-0.0339***	-0.0339***
			(0.0094)	(0.0110)	(0.0134)
election(t-3)				-0.0215**	-0.0144
				(0.0109)	(0.0131)
election(t-4)					-0.0002
					(0.0132)
year effects	yes	yes	yes	yes	yes
municipal effects	yes	yes	yes	yes	yes
observations	60,252	80,357	73,656	66,954	60,252

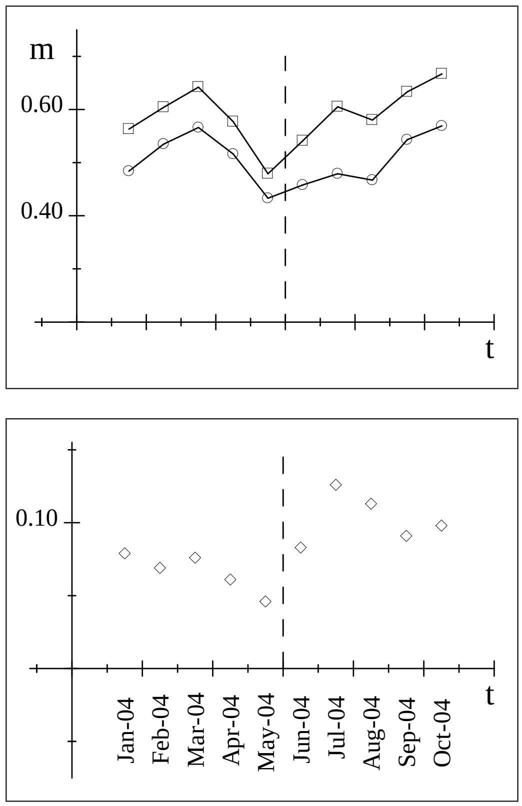
Notes: dependent variable = out-migration/in-migration (January 1^{st} to December 31^{st}) as a percentage of resident population (January 1^{st}). Within-group estimates. Standard errors clustered by municipality in parentheses. ***: p-value $<$ 0.01; **: p-value $<$ 0.05; *: p-value $<$ 0.10.

Table 9 Fiscal policy & migration: IV estimation

	out-migration	in-migration
budget surplus	3.174**	-3.309*
	(1.459)	(1.947)
income tax rate	4.730*	-7.939**
	(2.906)	(3.875)
Sargan test (p value)	0.72	0.22
year effects	yes	yes
municipal effects	yes	yes
observations	27,656	27,656

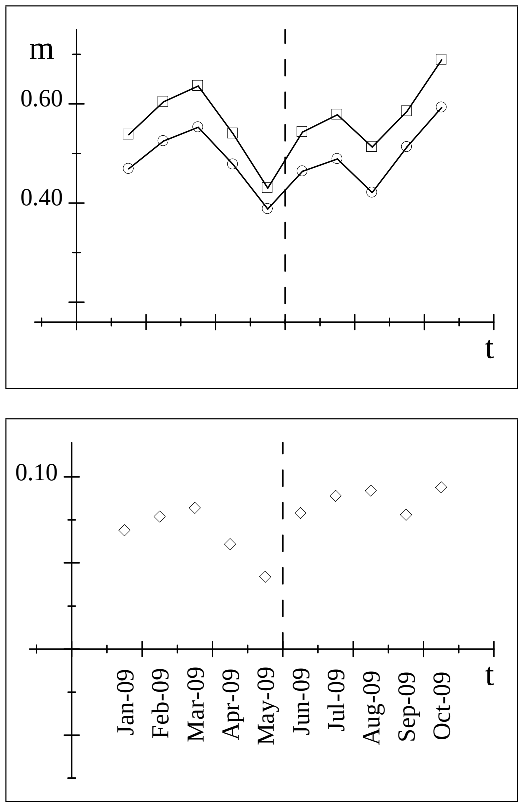
Notes: dependent variable = yearly (out-migration; in-migration) rate (%); Instrumental variables estimation; instruments: lagged election year dummies t-1, t-2, t-3, t-4; Standard errors clustered by municipality in parentheses; ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Figure 1 Elections and monthly migration rates (2004)



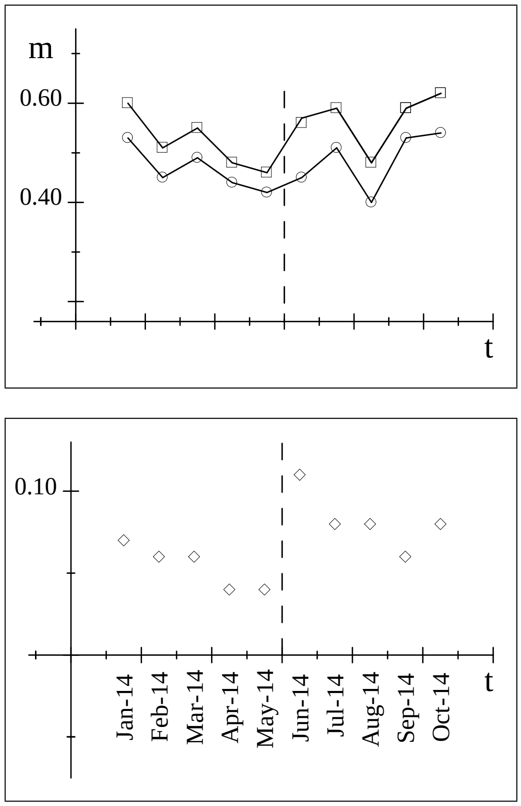
Notes: Average difference (◊) in total monthly migration rates (in-migration+out-migration) between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (◯) in 2004 (June, 12^{th}).

Figure 2 Elections and monthly migration rates (2009)



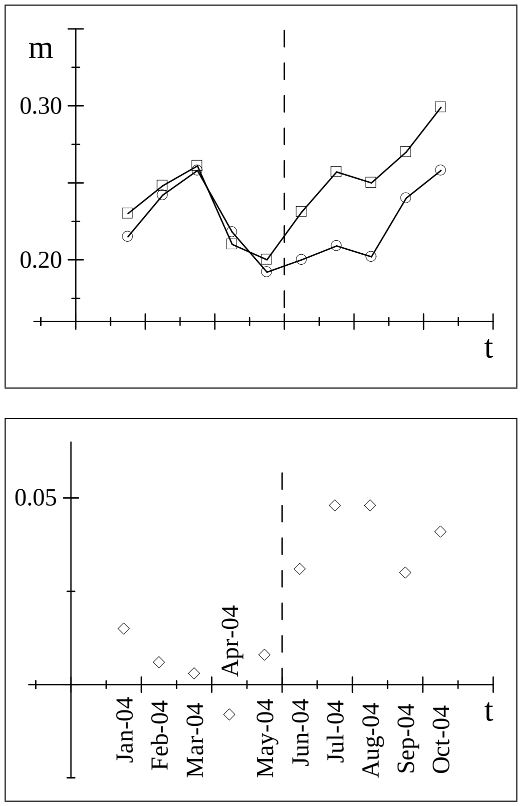
Notes: Average difference (◊) in total monthly migration rates (in-migration+out-migration) between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (○) in 2009 (June, 7^{th}).

Figure 3 Elections and monthly migration rates (2014)



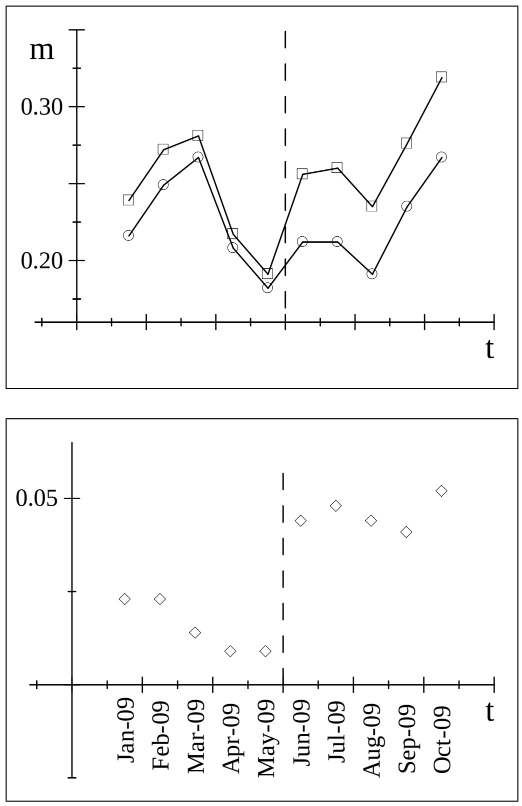
Notes: Average difference (◊) in total monthly migration rates (in-migration+out-migration) between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (○) in 2014 (May, 25th).

Figure 4 Elections and monthly out-migration rates (2004)



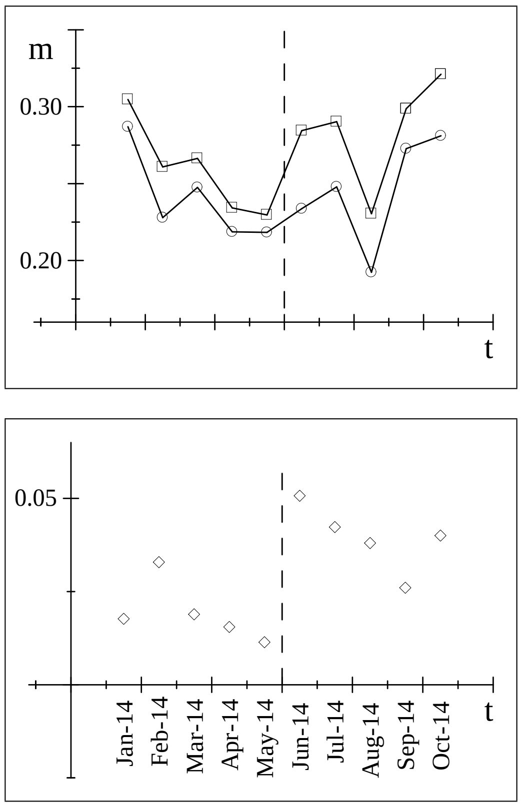
Notes: Average difference (◇) in monthly out-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (◯) in 2004 (June, 12^{th}).

Figure 5 Elections and monthly out-migration rates (2009)



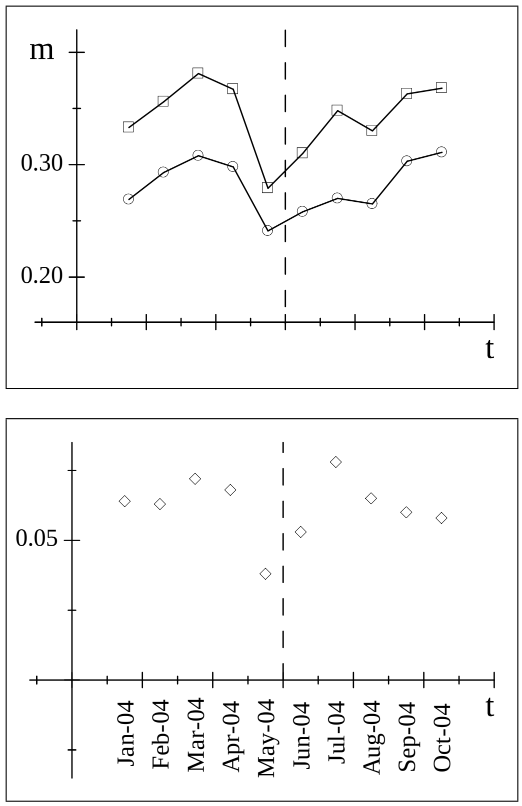
Notes: Average difference (◊) in monthly out-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (◯) in 2009 (June, 7^{th}).

Figure 6 Elections and monthly out-migration rates (2014)



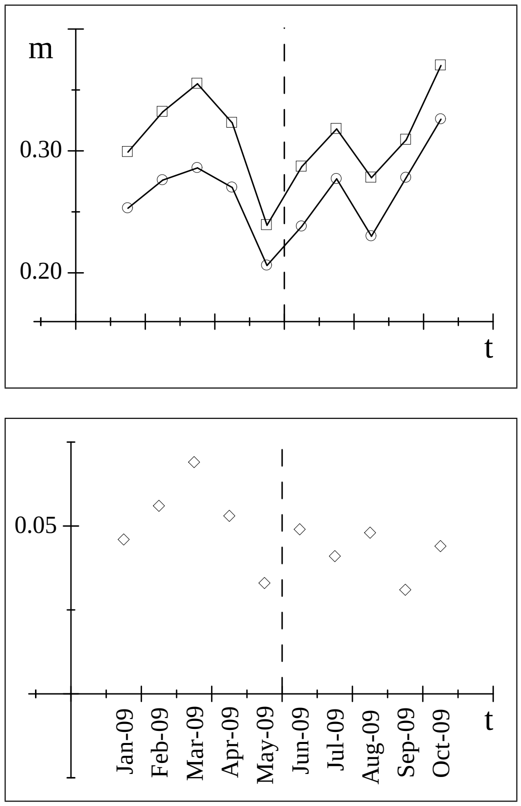
Notes: Average difference (◊) in monthly out-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (◯) in 2014 (May, 25^{th}).

Figure 7 Elections and monthly in-migration rates (2004)



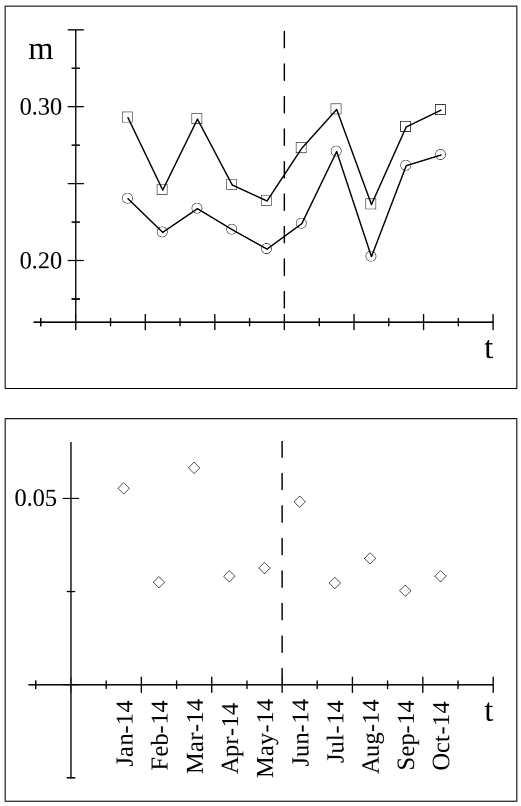
Notes: Average difference (◊) in monthly in-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (○) in 2004 (June, 12^{th}).

Figure 8 Elections and monthly in-migration rates (2009)



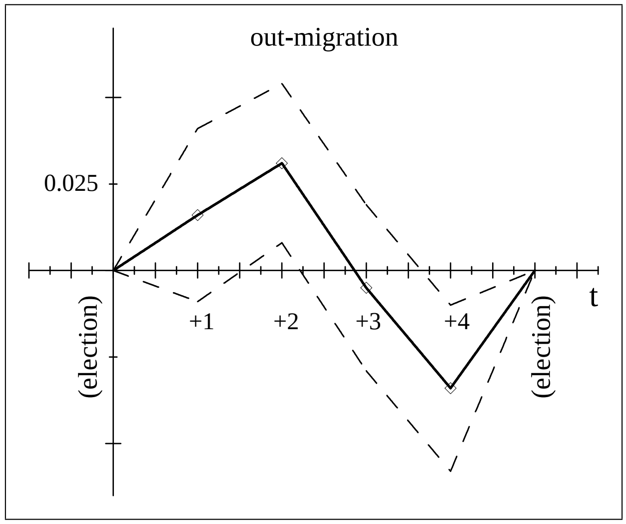
Notes: Average difference (◊) in monthly in-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (○) in 2009 (June, 7^{th}).

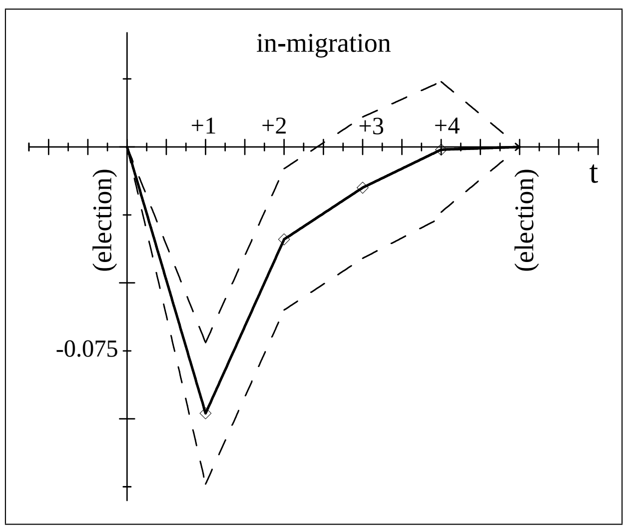
Figure 9 Elections and monthly in-migration rates (2014)



Notes: Average difference (◊) in monthly in-migration rates between municipalities holding mayoral elections (□) and municipalities not holding mayoral elections (◯) in 2014 (May, 25^{th}).

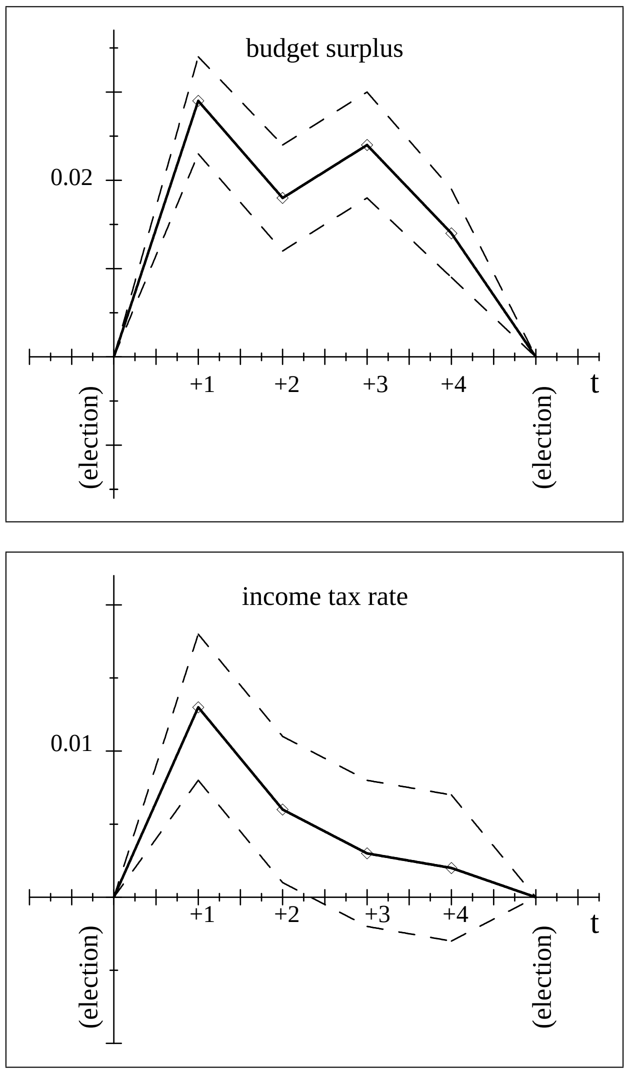
Figure 10 The electoral migration cycle: out-migration & in-migration rates





Notes: estimated impact of the timing of municipal elections on annual out-migration rates and in-migration rates in each of the four years between two elections [estimates from table 8, last column; 60,252 observations (2002-2014)]; dashed lines: 95% confidence interval.

Figure 11 The political budget cycle: budget surplus & local income tax rate



Notes: estimated impact of the timing of municipal elections on the budget surplus/deficit as a percentage of total revenues and on the local income tax rate set by municipalities in each of the four years between two elections (years 2006-2014); dashed lines: 95% confidence interval.

Appendix A Robustness check results

Length of the electoral cycle

As far as the election round of 2009 is concerned, 4,007 out of the 4,081 authorities holding elections had the previous election exactly five years earlier, in 2004, thus following a regular election cycle. As for 2014, 3,672 of the 3,677 authorities had the previous election in 2009, while I do not have complete information on earlier elections than the 2004 one. I report the estimates of coefficient β from equation (1) for the observations having regular electoral cycles in the years 2009 and 2014 in table A.1 below.

Table A.1 Sensitivity analysis: regular five-years terms only

	2009	2014
$\widehat{\beta}$	0.0187^{***}	0.0133^{***}
	(0.0055)	(0.0052)
obs.	66,280	66,300

Notes: dependent variable = monthly migration rate (%). Month and municipality fixed effects included. Standard errors clustered by municipality in parentheses. ***: p-value $<$ 0.01; **: p-value $<$ 0.05; *: p-value $<$ 0.10.

Observation window around the election

Table A.2 reports the results of estimation of coefficient β from equation (1) when using observations from 2 months (May-June) to 8 months (February-September) around the date of each of the three elections. Table A.3 reports the results of estimation of the same specifications using the alternative treatment group represented by the authorities having elections in the years 2003, 2008, and 2013.

Table A.2 Sensitivity analysis: alternative time-windows

	2004	2009	2014
$\widehat{\beta}$	0.0146	0.0358***	0.0571***
[May-Jun]	(0.0124)	(0.0109)	(0.0112)
obs.	13,402	13,404	13,270
$\widehat{\beta}$	0.0403***	0.0312***	0.0410***
[Apr-Jul]	(0.0092)	(0.0083)	(0.0084)
obs.	26,804	26,808	26,540
$\widehat{\beta}$	0.0395***	0.0237***	0.0256***
[Mar-Aug]	(0.0077)	(0.0070)	(0.0066)
obs.	40,206	40,212	39,810
$\widehat{\beta}$	0.0333***	0.0157**	0.0169***
[Feb-Sep]	(0.0068)	(0.0060)	(0.0059)
obs.	53,608	53,616	53,080

Notes: dependent variable = monthly migration rate (%). Month and municipality fixed effects included. Standard errors clustered by municipality in parentheses. ***: p-value < 0.01; **: p-value < 0.05; *: p-value < 0.10.

Table A.3 Sensitivity analysis: alternative time-windows & treatment group

	2003	2008	2013
$\widehat{\beta}$	0.0635*	0.0802***	0.0809***
[2 months]	(0.0365)	(0.0234)	(0.0277)
obs.	4,764	5,242	6,044
$\widehat{\beta}$	0.0744***	0.0616***	0.0843***
[4 months]	(0.0227)	(0.0160)	(0.0229)
obs.	9,528	10,484	12,088
$\widehat{\beta}$	0.0277	0.0228	0.0527**
[6 months]	(0.0189)	(0.0148)	(0.0208)
obs.	14,292	15,726	18,132
$\widehat{\beta}$	0.0102	0.0150	0.0293*
[8 months]	(0.0158)	(0.0126)	(0.0174)
obs.	19,056	20,968	24,176

Notes: dependent variable = monthly migration rate (%). Month and municipality fixed effects included. Standard errors clustered by municipality in parentheses. ***: p-value $<$ 0.01; **: p-value $<$ 0.05; *: p-value $<$ 0.10.